

LV.—*Some Alleged Cases of Misrepresentation.*

By F. A. BATHER.

MESSRS. C. WACHSMUTH AND F. SPRINGER have recently published an important theoretical paper \* in which they do me the honour to discuss at length certain arguments concerning the anal plates of the Fistulate Crinoids that I advanced in this Magazine † a year ago. In this new paper they put forward views so different to those which they appear to have hitherto held concerning the homologies of various plates in the Crinoid calyx, that to reconsider the anal plates alone would no longer be possible, while a scientific treatment of the question would involve one in a very lengthy and far-reaching discussion. This discussion, the inevitableness of which I foresaw when writing the paper referred to, though I hardly guessed the turn it would take, is better deferred until the description of the Swedish and British Fistulata has been accomplished with some attempt at accuracy. By that time it is quite possible that my American friends may have again changed their front, while I shall certainly be surprised if my own ideas have not undergone some modification. For the present I wish merely to defend myself against certain accusations which, though they have no connexion with the truth or falsity of any theory, could not fail, if left unanswered, to damage my scientific reputation in the eyes of those who have not time to go fully into the subject.

On p. 325 of my paper on the classification of the Inadunata Fistulata, after giving an abstract of the controversy regarding the anal plate, I said: "The history of this controversy is curiously full of misunderstandings and misrepresentations. I hope that I have made no such mistakes: I have done my best to avoid them." Knowing the great pains that I took in the matter, I the more deeply regret to learn that, in the opinion of Messrs. Wachsmuth and Springer, my references to their writings were "inaccurate" and my representations of their views "astonishing," "faulty," and "ridiculous." I am sorry, but not altogether surprised, and I console myself with

\* "On the Perisomic Plates of the Crinoids," Proc. Acad. Nat. Sci. Philadelphia, vol. for 1890, Part III. pp. 345-392. Published Feb. 1891. This paper is reviewed in the May number of the 'Geological Magazine,' 1891.

† "British Fossil Crinoids.—II. The Classification of the Inadunata Fistulata," Ann. & Mag. Nat. Hist. [6] v. pp. 310-334 and 373-486, April and May, 1890.

the thought that I am not the only person who has failed to grasp the meaning of these learned rather than lucid writers.

At the same time, on carefully comparing my account with their writings in the light of their recent criticism, I must confess, at the risk of exposing my dulness, that I cannot see very much to alter. I quite understand that the present ideas of Messrs. Wachsmuth and Springer are by no means those which I have attributed to them; but the question is not what they think, or even what they thought (or think they thought), but what they said, and what could be logically inferred from their statements. Let us then take their objections in order.

On p. 322 of my paper I gave certain extracts from their paper "On *Hybocrinus*, *Hoplocrinus* and *Baerocrinus*"\*, and I said, "In this paper then the authors consider the 'azygos' plate to be an independent morphological element of the dorsal cup, *not* a modified radial." On this Messrs. Wachsmuth and Springer remark (p. 389) "We know of no passage in that paper from which Bather would be entitled to draw any such inferences . . . he should have quoted the exact language, and give [*sic*] the page where it occurs." Let it be noted that my statement was introduced as an inference from various passages, and that I did quote the exact language of those passages so far as seemed necessary. Now, however, I will quote more fully from their paper on *Hybocrinus* &c., giving the page, and will, for the benefit of Messrs. Wachsmuth and Springer, indicate the various stages of my argument.

P. 376, footnote. "In Revision I, pp. 65-75, we considered the combined right posterior radial and the azygous plate in *Dendrocrinus*, which in their position and proportions resemble the right posterior radial in *Cyathocrinus*, to be a compound radial. At that time we thought that the second, the so-called azygous plate, in *Dendrocrinus*, *Homocrinus*, and in the *Cyathocrinidæ* generally, was a modified radial, and also that the anal tube, possibly, had been developed from an arm. Upon these points we were evidently in error."

*Conclusion.* Wachsmuth and Springer think that the "azygous" plate in the *Cyathocrinidæ* is neither a modified radial nor part of a compound radial.

What then is it?

P. 368, lines 8-12. ". . . we hope to prove further on that the plates which constitute the azygous side, both special

\* Amer. Journ. Sci. [3] xxvi. pp. 365-377, Newhaven, Nov. 1883.

anal plates and adjoining radial, had a common origin in all these genera, and were gradually evolved from a simple azygos plate."

*Conclusion.* (a) Wachsmuth and Springer think that an azygos plate existed in the dorsal cup of the *Fistulata* (to which group the context shows they are referring) before either the special anal [ $x$ ] or the right posterior radial.

(β) Wachsmuth and Springer think that from this azygos plate both the special anal [ $x$ ] and the right posterior radial were derived.

But is this azygos plate homologous with the azygos plate of *Dendrocrinus* and the *Cyathocrinidæ* generally?

P. 375. Figures 1, 2, 3, 4, 5, 6, 8, 9, representing "the arrangement of the plates of the azygos side in" *Baerocrinus*, *Hoplocrinus*, *Hybocrinus*, *Iocrinus*, *Dendrocrinus*, *Homocrinus*, *Poteriocrinus*, and *Eupachycrinus*. In each of these occurs a plate marked *a*. "*a*, azygos plate."

*Conclusion.* Wachsmuth and Springer consider that the azygos plate of *Baerocrinus* is homologous with that of other *Fistulata*.

P. 374, last par., continued on p. 375. This paragraph, which is really too long to quote in full, explains how the "large undivided azygos plate" of *Baerocrinus* "was gradually absorbed by the radial," *i. e.* right posterior radial, which in *Baerocrinus* itself "is not developed." This produces *Hoplocrinus*. In *Hybocrinus* the radial "has absorbed a greater portion," and "the upper left corner of the azygos plate has become divided off into a special anal plate."

*Conclusion.* (a) The azygos plate of *Baerocrinus* is the "simple azygos plate" of p. 368, from which the special anal and the right posterior radial were gradually evolved.

(β) Wachsmuth and Springer take *Baerocrinus* as the ancestral form, primitive in regard to its posterior side.

*Summary of Wachsmuth and Springer's views.*—There is in the *Fistulata* a plate not radial in origin, azygos in position, more conspicuous in the earlier forms; a plate that exists when even the radials are not fully developed, and from which another anal plate and a radial are evolved. So far then as the *Fistulata* are concerned this "azygos" plate is a primitive, independent morphological element of the dorsal cup.

This is the *rational* conclusion of a perfectly consistent hypothesis. But it is a conclusion which, when pointed out, does more than anything else to show the worthlessness of the assumptions on which it is based. Messrs. Wachsmuth and Springer are now as much astonished at it as I was, and

I am fully prepared to admit that they meant nothing of the kind; but language, not thought-transference, is the only recognized medium of scientific communication.

To return to my paper. On p. 324, in summing up the position which Wachsmuth and Springer held in 1886, I said: "(1) Azygos plate (Az) a primitive element of dorsal cup." On this they say (p. 390) "A careful examination of both sections of Pt. III of the Revision, will show nothing to justify Bather in assuming that we regarded the Azygos as a 'primitive element.' We only stated on p. 11: 'the lower segments (of the compound radials) are probably embryonic plates, which were resorbed by the upper segments.'"

My meaning was quite clearly explained on p. 323. In their own words [Revision III. (p. 12), Proc. 1885, p. 234] "the azygos piece may represent the lower segment of the posterior radial;" but [Rev. III. (p. 11), Proc. 1885, p. 233] "the lower segments are probably embryonal plates." For the rest they repeat in 1885-6 what they said in 1883, adding [*Pag. cit.*, footnote] "For further information on *Baerocrinus* and the gradual resorption of the azygos and anal plate in the Inadunata generally, we direct attention to our paper on *Hybocrinus*, *Hoplocrinus* and *Baerocrinus*."

Now a structure that is "embryonal" or (as they now prefer to quote) "embryonic" is usually regarded as primitive or ancestral. Certainly it is so regarded when there is nothing said to the contrary, and when it is more highly developed the earlier the form. It was therefore natural to suppose that Wachsmuth and Springer regarded the Azygos plate as an ancestral or primitive structure; and when I found that on the question of the evolution they still stood by their previous paper, I had no hesitation in stating this conclusion.

It really seems to me, now that I read Messrs. Wachsmuth and Springer's protest, that they must attach to the word "primitive" some sense with which I am not yet acquainted.

Next I said (p. 324):—" (2) Anal (x) and right posterior radial derived from azygos plate." This Messrs. Wachsmuth and Springer (p. 390) regard as "equally inaccurate." But if I had said "derived from the *undivided Azygos in Baerocrinus*" I should have expressed their views.

Since, however, the "azygos" of *Baerocrinus* is admittedly homologous with the azygos plate of other *Fistulata*, I fail to see where the difference comes in.

In 1886 Messrs. Wachsmuth and Springer wrote as follows

[Rev. III. (p. 196), Proc. 1886, p. 120]: "it is probable that one of the non-armbearing so-called radials [in *Baerocrinus*] represents an azygous plate, such as we find in most of the *Fistulata*, that the right posterior radial and the anal plate were as yet undeveloped, and that *Baerocrinus* had but four radials. This interpretation of the plates, it seems to us, is corroborated by the gradual disappearance of the azygous plate among allied forms in palæontological times, and by the contemporary increase in the dimensions of the right posterior radial and the anal plate. The two latter pieces were absorbed from the azygous plate: at first the posterior radial, which in *Hoplocrinus* took the right upper corner, the left side remaining intact; afterwards in *Hybocrinus* the anal piece, which absorbed the left corner of the plate also." This seems quite clear; the azygos plate is absorbed in *Hoplocrinus* and *Hybocrinus*.

But the footnote on the same page is even clearer. The anal of *Antedon* and the azygos of *Baerocrinus* "both agree . . . in being absorbed by other plates; the azygous plate palæontologically by the right posterior radial and anal plate, the other in the growing animal over the whole surface." Now this means that in the evolution of the *Fistulata* the plate in the successive genera homologous with the azygos of *Baerocrinus* was gradually absorbed by the radial and anal.

But why mention *Baerocrinus* at all?

On p. 40 of Revision III. (Proc. 1885, p. 262) Wachsmuth and Springer say "In our chapter on the radials we have already alluded to the azygous piece, and expressed our conviction that its gradual resorption gave origin, not only to the right posterior radial, but also to the anal plate." Why, let me ask Messrs. Wachsmuth and Springer, did they omit all reference to *Baerocrinus* in this passage? Presumably because this perpetual insertion of the name *Baerocrinus* would make nonsense; for they cannot mean to say that the anal of the Carboniferous *Scaphiocrinus iowensis* has absorbed part of the azygos of the Ordovician *Baerocrinus Ungerni*. When did it cross the Atlantic to collect fossils in the *Brand-schiefer* of Erras?

Again I said (p. 324): "(3) Anal of *Antedon* not homologous with any plate of the *Fistulata* but an embryonic inter-radial." This statement of their views is they say (p. 390) "more faulty yet. To agree with Pt. III of the Revision it should be amended as follows: Anal plate of *Antedon*



larva homologous with plate  $x$  of the *Fistulata*, and interrarial in position."

I am willing to admit that their present statement is quite consistent with Part III. of the Revision, and had they chosen to say as much in that work I should not have been led astray by the following considerations.

In their paper on *Hybocrinus*, *Hoplocrinus* and *Baerocrinus*, p. 377, they said "the 'anal' plate of the young *Antedon* is evidently not the homologue of the plate in the *Cyathocrinidæ* which we have designated as the 'special' anal plate, but . . . it is the equivalent of the undivided azygous plate in *Baerocrinus* and *Hoplocrinus*." On this they subsequently remarked [Revision III. (p. 196), footnote; Proc. 1886, p. 120] "In making this statement we had overlooked the fact that the latter plate\* is simply an interrarial with special function, while the azygous plate in *Baerocrinus* is as much radial as interrarial." If they had meant what now they say they meant, they should have taken this opportunity of stating that they then considered the anal plate of *Antedon* to be homologous with the special anal of the *Cyathocrinidæ*. That certainly was not what I inferred from the above-quoted footnote: for, I argued, if the azygos of *Baerocrinus* is as much radial as interrarial, so also is the special anal plate that was once a part of it; but the anal of *Antedon* is *simply an interrarial*, therefore it cannot be homologous with the special anal of the *Cyathocrinidæ*.

In an earlier part of Revision III. (p. 39) published in 1885 (Proc. p. 261) they had laid some emphasis on the distinction between "interrarials" and "the one true anal plate," and, although it is quite true that they compared the various positions assumed during growth by the anal of *Antedon* with the positions occupied in the evolutionary series by the anal plate of the *Fistulata*, still they never definitely stated the homology.

Even the sentence which they now (p. 390) quote from Rev. III. p. 40, that "at last in *Cyathocrinus* the latter plate [Azygos] was entirely removed, and the anal plate took the position of that in the larva of *Antedon*," does not necessarily imply homology; had they said "the anal plate took the position that it occupies in the larva of *Antedon*," this would have shown that they considered the two plates homologous. I, reading the sentence in the light of their subsequent footnote, naturally supposed that the ambiguity of its wording was intentional.

\* Anal of *Antedon* larva.

I have already alluded \* to my omission on p. 323 of the words "in *Baerocrinus*" from their statement that the Azygos plate is as much radial as interrarial, but Messrs. Wachsmuth and Springer will not accept my explanation. They now say (p. 390), "We stated correctly that the 'Azygos of *Baerocrinus* is neither radial nor interrarial' for it rests between two radials and alternates with the basals; but to say the same thing of *Homocrinus*, *Dendrocrinus*, etc. would be ridiculous."

Whether correctly or no, Messrs. Wachsmuth and Springer never did use the words which they have here put between inverted commas, but they used the same words as I used, although I did not put them between commas. It is odd, by the way, that they should misquote themselves three times on one page.

They were (in 1886) contrasting the anal of *Antedon* with the azygos of *Baerocrinus*; the former they said was simply an interrarial, the latter as much radial as interrarial. Remembering that only three years before they had dropped the radial origin of this azygos plate, they now wished to correct themselves; consequently the important point in the 1886 statement seemed to be the partly radial position of the azygos plate in *Baerocrinus*. But they continued to speak about the palæontological history of that plate, calling it merely the azygos plate. In this latter half of the paragraph, as I have pointed out, they extended the term to all *Fistulata*. I naturally supposed that if there were any importance in this partly radial position of the azygos in *Baerocrinus*, it lay in the fact that the azygos as a morphological entity was partly radial in position. That I was right in my supposition is proved by various passages in the present paper, where they lay stress on the fact that the azygos plate invariably alternates with the basals.

As to the point that it would be ridiculous to say the same thing of *Homocrinus* and *Dendrocrinus*, I reply that it is ridiculous to say that this plate is interrarial in *Baerocrinus*; it is only interrarial in the same sense as that in which any radial may be said so to be. If, however, it could ever be correctly called interrarial, so could the azygos plates of *Hoplocrinus* and *Ilybocrinus*, and where exactly the line should be drawn I do not see.

I therefore maintain that I was justified, when summarizing, in the omission of special reference to *Baerocrinus*.

\* Ann. & Mag. Nat. Hist. [6] v. p. 486.

I hope that I have now successfully defended myself against the charges of misrepresentation, though I may not be acquitted of obtuseness. There still remain, however, a few points in the body of Messrs. Wachsmuth and Springer's paper to which I must regretfully take some exception.

On p. 377 they say of me "He agrees with us and Carpenter that the radial anal plate, the so-called azygous piece, constitutes primarily the lower portion of the right posterior radial, which in the earlier forms occupies a position immediately below the radial." This represents with perfect accuracy the view given in my paper; it represents I believe the view of Dr. Carpenter; it may, for all any one can tell, represent the present view of Messrs. Wachsmuth and Springer;—but I deny that it represents their views of 1883-5-6, which were the last that had appeared when I published. According to those views the earlier forms were *Baerocrinus*, *Hoplocrinus*, and *Hybocrinus*; but in *Baerocrinus* there was, they said, no right posterior radial at all; while in the other two the radianal is certainly not immediately below the radial. This difference was all-important from my point of view, and if Messrs. Wachsmuth and Springer *now agree with me* I am glad to hear it, but they have come to the opinion of Carpenter and myself, not I to theirs.

On p. 380 they say "Mr. Bather assumes, as before stated, that the anal plate, the plate *x*, is derived primitively from a brachial &c." I should not venture to *assume* anything so important; my *conclusion* was arrived at after eleven pages of discussion and argument. The essential part of my conclusion was that the plate *x* passed down into the dorsal cup from above; the idea that it was derived from a brachial and the name "Brachianal" followed as corollaries, but nothing depended on them in the subsequent discussion as to Phylogeny and Classification.

On p. 381 Messrs. Wachsmuth and Springer say "We must also protest against his statement on p. 324. There, in summarizing our position on the anal question, he says under *Iocrinus*: 'Radial growing larger at expense of Azygos, and here has *absorbed x*;' while the fact is we have always held, and have said so, that this plate *x* was *unrepresented* in *Iocrinus* and was as yet *undeveloped*' \*." In reply to this I need only refer Messrs. Wachsmuth and Springer to their own paper on "*Hybocrinus*, *Hoplocrinus*, &c.," p. 370, second paragraph, line 15. Here, on the subject of *Iocrinus*, they

\* The italics are Wachsmuth and Springer's, *not* mine.



write: "We admit that the so-called postero-lateral radial (fig. 4 a) is not articulated to the bifurcating plates [*sic*] but united with it by suture; we insist, however, that the latter cannot be an azygous plate, as suggested by Carpenter,—nor is it a brachial—but that it is the equivalent of the combined small radial, and small anal plate in *Hybocrinus*." Again, *op. cit.* p. 376, first paragraph, "In *Iocrinus* . . . we suggest that possibly the radial may embrace an undivided anal piece." Whether they suggested or whether they insisted is immaterial; but if they did not mean that the anal  $x$  was represented in the right posterior radial, what that can be expressed by language did they mean?

On p. 383 they say "We cannot understand how Bather on p. 330 of his paper could conclude from the structure of *Ectenocrinus*, which he has regarded as one of the most 'primitive forms,' that  $x$  'originated as a plate morphologically corresponding to an ordinary brachial.'" Now *Ectenocrinus* is not mentioned on p. 330 of my paper; the genera adduced are *Iocrinus* and *Merocrinus* and, in a less degree, *Heterocrinus*. On p. 379 of the same paper it is argued that *Iocrinus* and *Merocrinus* are more ancestral than *Heterocrinus*, and *Heterocrinus* than *Ectenocrinus*. "Comment," says the critic, "is needless!"

On pp. 384–5 they criticise my expression "the shifting of the radianal," and I agree with their criticism; but they might have alluded to the fact that on p. 78 of Revision I. (Proc. 1879, p. 301) it is stated that in *Homocrinus* "the lower portion of the compound plate is pushed slightly to the rear," and that on p. 40 of Revision III. (Proc. 1885, p. 262) is written "In *Poteriocrinus*, *Eupachycrinus* and *Zeacrinus* the azygous plate is . . . completely pushed out of the radial position which it had previously occupied." But no doubt they did not mean this when they wrote it.

On pp. 383 and 386 they ascribe to me some "theory" that "the ventral sac represented a modified arm." So far as I am aware, the only people that have ever held this theory have been Messrs. Wachsmuth and Springer themselves; and of it I said (p. 331) "this view is as unnecessary as it is untenable."

In conclusion, I trust that no readers of this defence will suppose that the theories of Messrs. Wachsmuth and Springer are in the smallest degree invalidated by it. It is just because human nature is so apt to substitute personality for abstract truth, and to be prejudiced by quite unessential but distracting details, that I have thought it advisable to treat these disturbing questions apart from the real discussion.

When time is ripe for that, I venture to hope that it will not be necessary for my very friendly antagonists to lay so much stress upon misrepresentation.

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LVI.—*Insect-Larva (Cecidomyia, sp.) eating Rust on Wheat and Flax.* By N. A. COBB and A. SIDNEY OLLIFF\*.

ON many specimens of rusted wheat received from various parts of New South Wales we have noticed an orange-coloured larva. Our attention was first called particularly to these larvæ by the fact that they were invariably more common on the rusted plants. The orange colour of the larger of these larvæ would naturally suggest at once some connexion between them and the rust, which is also orange-coloured. This, in fact, had already been the case, one farmer averring most positively that these larvæ were the cause of the rust. This conclusion, founded on colour resemblance alone, could have little, in fact almost no weight, and we were inclined to regard the colour as deceptive, like the red coloration on fence-rails, and felt ourselves fortified in that position by the knowledge that these larvæ were probably *Cecidomyia* larvæ and would very likely be found to live on the juices of the wheat-plant. Later, however, specimens of rusted linseed were received, and on these also the same orange-coloured larvæ were found. We say the same, because on placing them side by side with larvæ from rusted wheat we could detect no difference. If these larvæ fed on the juices of plants, it was somewhat remarkable that the same species should be found on such different plants as wheat and flax. On the other hand, both these plants, though widely different from each other, were attacked by a rust in its *Uredo*-stage, and the *Uredospores of the rust were very similar*. This fact led to the suspicion that the rust-spores might be the food of the larvæ and to the following experiment. A moist chamber was partly filled with water, and in the midst of the water a piece of lead was so arranged as to form a miniature island about one fourth of an inch across. A fresh cutting was then taken from a wheat-leaf in such a manner as to include on its surface a single *Uredo* sorus. This cutting, one eighth of an inch wide and one quarter of an inch long, was placed on the miniature island together with three larvæ of the *Cecidomyia*. The larvæ were taken from a rusted linseed plant, and pains

\* From an advance proof, communicated by the Authors, from the 'Agricultural Gazette of New South Wales,' vol. ii. part 2. By authority. Sydney, 1891.